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VOL. LXI MAY 1, 1925 No. 1583

CONTENTS

The Growing Opportunities for Medical Research: DR. LEWELLYS F. BARKER 449

The American Association for the Advancement of Science:

The Trend of Thought in Physics: PROFESSOR W. F. G. SWANN 452

The White Indians of the San Blas and Darien: DR. REGINALD G. HARRIS 460

The James-Simpson-Roosevelt Expedition of the Field Museum of Natural History: DR. WILFRED H. OSGOOD 461

Scientific Events:

The International Congress of Forestry; Work of the Northeastern Forest Experiment Station; Sigma Xi and the California Institute of Technology; The Third National Colloid Symposium; Townshend Stith Brandegee 462

Scientific Notes and News 464

University and Educational Notes 467

Discussion and Correspondence:

Series in the Arc Spectrum of Nitrogen: DR. C. C. KIESS. *The Undertow:* DR. T. T. QUIRKE. *Origin of the Loess of the Palouse Region, Washington:* RAY C. TREASHER. *The Stem of Magnolia as a Laboratory Type:* A. W. HAUPT 468

Scientific Books:

Bliss on the Calculus of Variations: DR. F. D. MURNAGHAN 470

The Norwegian Identity Book: X. 470

Special Articles:

A Highly Sensitive Physical Method for detecting Proteins in a Solution: DR. LECOMTE DU NOUY 472

Science News x

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THE GROWING OPPORTUNITIES FOR MEDICAL RESEARCH[1]

IT is related of the rugged English surgeon, John Abernethy, that on entering the lecture hall at the beginning of a new medical session he once exclaimed "Good God! What will become of you all?" An attractive and persuasive lecturer, Abernethy gained no distinction in original research; indeed, his dogmatic attitude is said to have been repressive rather than stimulative of original inquiries. An attempt to answer the question Abernethy asked was, later on, made by another British surgeon of entirely different stamp, namely, Sir James Paget, an eloquent lecturer who besides was a truly great investigator of pathological and surgical problems, one who cherished the ideal of uniting scientific studies with practical activities throughout his professional life. He made some statistical studies to determine what actually did become of the students whom he taught in the medical school, and his conclusion was that, as a general rule (with but few exceptions), "such as the student had been, such was the practitioner." The validity of this rule was certainly demonstrated in the case of Paget himself, for, in his first winter session in the medical school, while working in the dissecting room, he found by means of a pocket lens that certain specks in the voluntary muscles were parasitic worms and not, as earlier observers had thought, spicules of bone; as a result of his observations a piece of the muscles containing the specks was sent to R. Owen, who confirmed the discovery and gave to the worm its scientific name, *Trichina spiralis*. Young Paget, who showed his ability to discover new things in his first year in the medical school, later on became, as you know, so great a pathologist that he rose to a place in England comparable with that occupied by Rudolph Virchow in Germany and so great a surgeon that he is always numbered among surgeons of the first rank. His name, you will recall, is attached permanently to a well-known disease of the breast and to an equally well-known disease of the bones.

This morning it was my good fortune to be present during a part of the session when students of this school, members of your Undergraduate Medical Association, were reporting some of the original observations and experiments that they had carried on

[1] An address delivered to the Undergraduate Medical Association of the University of Pennsylvania, Friday, April 3, 1925.

during the past year. The reports of such researches augur well, I believe, for the future of those who conducted them. For, notwithstanding the contrary opinion held by some medical educators, I am convinced that those who learn how to observe and to think for themselves during their undergraduate careers are more likely to be successful, in later life, in teaching, in practice and in research, than are those who are content merely to acquaint themselves with the observations and reflections of other men. During the period when I was in charge of the teaching in the medical clinic of the Johns Hopkins Medical School I asked each fourth year student to undertake some small piece of original work. In watching the subsequent careers of those students I have been deeply gratified to find how many of those who had shown the greatest industry and enthusiasm in that work have already become distinguished as practitioners, teachers or investigators. Nothing else has seemed to me to equal in importance, during a student's undergraduate period, the cultivation of the spirit and the methods of scientific inquiry. The mere learning of facts that have been accumulated by others is of far less significance than training in the method of science, recognizing the importance of the continual expansion of knowledge, learning how to participate in such expansion and acquiring a love for and the determination to pursue original studies. In this respect, my own observations as a teacher have been entirely in accord with those of Sir James Paget.

The scientific method of solving problems is now commonly resorted to by all productive workers, no matter what their special fields of work. The principles involved are relatively simple and have been clearly defined by Professor John Dewey in a little volume entitled "How We Think"; I have, myself, attempted to illustrate their value for the solution of problems of diagnosis in an article written for *Oxford Medicine* (Vol. I) under the title The Rationale of Clinical Diagnosis. When one is confronted by a difficulty, that is to say, when one has a problem to solve, the quickest and best way of overcoming the difficulty, of solving the problem, is to make a systematic attack upon it. Such a systematic attack includes: (1) the acquisition of data that will help to locate and to define the problem, (2) the arrangement of the data in various ways and brooding over them as thus arranged in order that suggestions of solution of the problem may occur to the mind, (3) the reasoning out in detail of the implications of each suggestion of solution that arises in consciousness, and (4) the thorough testing of such suggestions thus minutely worked out as to their implications by comparison with the actual data that have been accumulated, resorting to further observation and experiment to extend these data when necessary in order to determine the validity or the non-validity of the hypothetical suggestion of solution.

All these stages of the program must be passed through; they are essential serial steps in orderly problem solving. And if the investigator will keep them definitely in mind in planning his work and during his prosecution of the work not only will he save himself much time and trouble but he will also greatly increase the likelihood of his arriving at sound conclusions. No matter what the original task that one sets for himself, be it say the discovery of the mode of regeneration of peripheral nerves, or the devising of methods for the prevention of the ill-effects of an infection or an intoxication, or the ascertainment of the significance of the content of the blood in a given variety of ions, or the experimental production in an animal of a disease in which one is interested, the only satisfactory method of accomplishment lies in the process of accurate observation, of well-conceived experimentation for the refinement of observation, and of intensive and patient reflection, which I have just now referred to as the essential features of the method of science.

If you would enjoy reading a fuller account of these processes written by a master of the experimental method as applied in physiology and pathology, let me recommend to you most heartily the perusal of Claude Bernard's "Introduction à l'étude de la médecine expérimentale," which is fully as interesting to-day as it was when it was first published in 1865. Hitherto this valuable treatise has been available only in the French language, but I am glad to learn through Mr. Abraham Flexner that an English translation is shortly to be issued by the Macmillan Company. Do not overlook it!

The problems that present themselves for solution to those of us who work in medicine are infinite in number and in variety, but, in the last analysis, they all have a bearing upon (1) the nature and preservation of health and (2) the cure, the amelioration or the prevention of disease, in living organisms. A healthy organism is one that makes adequate responses to the environmental influences that impinge upon it; a diseased or an anomalous organism is one that exhibits inadequate responsivity to environmental influences. Obviously, therefore, our medical tasks include studies of the substances and forces within the organisms that react, studies of the various influences in their environment that stimulate to reaction, and studies of the reactions themselves as they occur.

What a developed living organism (or phenotype) is at any given time that it is studied would seem to depend (1) upon the substances and energies that

lay within it at the moment of its start and (2) all the changes that have occurred within it as a result of influences in the surroundings in which it has been immersed from the time of its start up to the moment of study. For human beings, this would appear to be true of the foetus, of the child, of the adolescent, of the mature individual and of the senescent. If we desire clearly to understand how men, women or children will respond to physical changes, to chemical changes, to psychological changes and to sociological changes in their environment, we can scarcely hope to do so without knowing much about how they have begun, much about how they have developed, and much about the influences to which they have been exposed and how they have reacted to those influences.

Without much knowledge of the inherited (genotypic) and environmental (paratypic) elements of a given constitution we are likely to be unable satisfactorily to understand the disposition or the non-disposition of the person possessing that constitution to the development of various disease processes, or to the exhibition of inadequate somatic or psychic responsivity to external stimulation. That is why in medicine investigators are no longer content to study the anatomy, the physiology and the pathology of developed organisms only, extremely important as such studies are, but feel that they must inquire also into the problems of inheritance on the one hand and of growth and development on the other. For in order to understand how living organisms have come to be what they are and why there are such variations in different organisms in their modes of response to apparently identical environmental stimuli genetic and comparative studies are indispensable. It is no accident that one person develops influenza and another does not, that of two persons suffering from typhoid fever one dies and another recovers, that one woman weighs two hundred and fifty pounds and her neighbor of the same age only eighty, that of two boys exposed to the same temptation one yields while the other resists, that of two members of a class one is a gold medallist and the other is "plucked," or that of three young women surrounded by similar emotional stimuli one shows elation, one depression and the third apathy. Medical men, however, are far, as you know, from being able, in every case, as yet to explain these diversities of response. I believe, however, that such divergent modes of reaction will prove to be susceptible of explanation and that as medical science progresses we shall gradually acquire the knowledge that will enable us to predict when, where and how disease will develop, and why some people think, feel and behave adequately and others inadequately. Gradually, too, with the increase in our powers of prediction we shall also see an augmentation of our power to control. During the past century the ability of medical men to predict and to control in the domain in which they work, has, it is true, grown enormously, but all will admit that this growth, gratifying as it is, must be regarded as infinitesimal compared with that to which our profession aspires.

The idea is at present very widespread that scientific work in medicine is and can be done only in laboratories and, particularly, in the laboratories devoted to the study of the preclinical sciences. That is, of course, a very grave error. Though the establishment of such scientific laboratories has tremendously extended and favored opportunities for original research bearing upon medical problems, nevertheless the work in our better medical and surgical clinics, not only in the laboratories attached to them but also at the bedside should be, can be, and, indeed, I believe is, just as truly scientific as that which is carried on in the laboratories of the preclinical branches.

The problems of the clinics and those of private practice differ from those of the preclinical institutes only in their greater diversity and complexity, and knowledge is now being advanced in the clinics and in the clinical subjects by means of precisely the same scientific methods of inquiry as are used in the more elementary branches of science. When there are problems to be solved the same principles of study apply anywhere and everywhere. Even in the busy life of a medical practitioner the condition of each patient met with ought to be looked upon as a special problem for solution and the work of solution of that problem will be the more effective the closer the principles of the method of science are adhered to. But, aside from this daily application of the scientific method it should not be forgotten that a number of most important discoveries in medicine have been made by general practitioners, busy men with fine brains, who, despite the pressure upon them of multitudinous activities, still found the time to engage intensively in the prosecution of original inquiries. As Sir James Paget once said, "none find so much time to spare as they that have too much to do and do it with all their might." Jenner's discovery of vaccination, Koch's early studies upon wound infection, Lister's work on antiseptics, Mackenzie's observations of cardiac irregularities and Banting's work on insulin, are shining examples of profitable research conducted by men in practice. And though our great institutes for experimental research and our laboratories and organized clinics in the medical schools offer extraordinary facilities for the conduct of original investigations, the time will never come when practitioners of medicine and surgery can afford to

neglect the opportunities that practice yields for accurate, original investigation or for the deliberate, logical reflection that helps to expand the confines of knowledge. Moreover, minute accretions to the store of knowledge are not to be despised; indeed, were it not for the continuous collection of such small increments fewer of the so-called great discoveries would be made, since for the most part great discoveries are nothing but the summits of structures composed of a mass of minor investigative results.

Men who continuously insist upon the cross-examination of the evidences of their senses, men who, when they observe an object or an event, try to consider also all the conditions in which that object or event is situated, men who examine into the variations, the concurrences, the sequences and the mutual relations of phenomena, men who are cautious not to insert their own beliefs and expectations and wishes into their observations, men who are so distrustful of their memories that they record accurately what they observe at the time and place of observation, men who are not satisfied merely with thinking of explanations but insist upon testing them in order to see whether or not they are true, are the men who discover new things, who detect errors in conceptions that are supposed to be well founded, and who, in general, do most to contribute to the advance of our knowledge of health and disease.

The growth of knowledge has often been compared to that of an avalanche; its magnitude and its velocity must steadily increase. Every single advance prepares the way for a whole series of other advances. Every new method devised makes it easier to invent still other modes of extending observation. The physician who died in 1900, could he come to life again, would be astonished and perhaps confused by the medicine of 1925; if any one of us were to stop his medical work to-day and were to try to resume it ten years from now he would doubtless find himself almost as disoriented in special medical fields as is a patient in his general surroundings when he suffers from Korsakoff's psychosis!

You who are members of this Undergraduate Medical Association enter upon your medical activities at the most favorable period the world has ever known for the enlargement of medical knowledge and for the advancement of human welfare. Just what part each of you who will graduate here (from an institution made famous by Cope, Leidy and a long series of original investigators) will play in this enlargement and this advancement remains to be seen. There will be many difficulties in the way; for an exaggerated account of them consult the recent clever novel "Arrowsmith" by Sinclair Lewis. But difficulties are only additional spurs to determined men. Some of you will have better brains than others but each of us has to do the best he can with the brain that he possesses. Some of you will be able to command more leisure for scientific studies than will others, but again every one can secure some leisure for scientific work if he will make his life orderly. Some of you may be privileged to work in institutes devoted entirely to original research; some of you will find it advantageous to combine research with teaching; the majority, perhaps, will engage in medical practice of one sort or another and will there find the special opportunities for certain kinds of scientific work that can not be found apart from such practice. No matter where your lot may be cast the main thing is that you utilize every opportunity that comes to you to observe accurately, to record carefully, to imagine vividly, to reason logically, to experiment cautiously and to test rigidly the validity of the explanatory ideas that occur to you. For these are not only pleasures that you may enjoy; they are also obligations and responsibilities that fall upon you as medical men. May I extend to each and every one of you my very best wishes for happiness, success and duty fulfilment in your work during the coming years.

LEWELLYS F. BARKER
THE JOHNS HOPKINS UNIVERSITY

THE TREND OF THOUGHT IN PHYSICS. II

STATIONARY STATES AND THE LAWS DESCRIBING ELECTRONIC MOTION

APART from the nature of emission of quanta during the transitions between the stationary states, there yet remains the problem of accounting for the stationary states themselves. Now the mind is content that the electron should not be allowed to move in any manner it pleases. It is content to have an equation of motion of the Newtonian form, for example, a simplification of the force equation of electrodynamics, for this case; but, having become accustomed to this, it resents any further conditions of constraint upon the motion, such as are involved in the Wilson-Sommerfeld conditions that the quantities $\int pdq$ shall be integral multiples of h, where the p's and q's are suitably chosen momenta and coordinates.

Now the question arises as to what attitude of mind it is necessary for us to get into in order that the existence of stationary states shall seem reasonable to us in terms of the criteria for reasonableness to which we adapt ourselves. This may well be considered as part of the whole question of the description of motion.

Suppose we return for a moment to the circuital

relations of electrodynamics with the ϱ and ϱu terms absent, *i.e.*, suppose we return to the equations for free space. The electron, in this scheme of relations, is the place where **E** and **H** cease to be analytic functions of x, y, z and t—crudely speaking—the places where **E** and **H** become infinite.

Now it is easy to see that if the values of **E** and **H** be assigned at all parts of space at any one instant, the equations determine their values everywhere for all time, and with these values the motions of the singularities themselves. No law other than the circuital relations is necessary to determine the complete subsequent history of the system, once the initial conditions have been stated. This point was emphasized by Sir Joseph Larmor many years ago. However, corresponding to any motion we like to assign to a singularity, we can always determine fields **E** and **H** to correspond. But the equations are linear so that the addition of two such solutions results in a field which is itself a solution. In other words, as far as these equations are concerned, there is no mutual influence between the electrons. An initial assignment of the fields will control the motions of the singularities for all time, and so their mutual relations; but there is nothing in the equations themselves to prohibit any motion whatever. In fact, the scheme is too wide for our universe. The real function of such an equation as the force equation of electrodynamics is to restrict matters so that, of all the fields which might be possible with a sufficiently arbitrary choice of initial conditions, only those to be found in our universe are the subjects of discussion. In this sense, the force equation and even the law of gravitation constitute what, many years ago[10] I termed subsidiary laws, *i.e.*, laws which would be unnecessary, and indeed superfluous if we knew the initial conditions, but which serve to fill the gap caused by our ignorance of those conditions, by restricting the cases we are to regard as possible to those which could have evolved out of the actual initial conditions inherent in our universe.

Now it is possible to utilize this principle of adding additional conditions to still further restrict the atomic orbits beyond the restrictions implied in the laws of classical dynamics. To be explicit, suppose that, confining ourselves for the time being to circular orbits, we write down the ordinary equations of classical dynamics, and work out the radius, a, of the orbit in terms of the angular momentum p, for an electron of mass m revolving about a nucleus of charge E. It is easy to show that

$$a = \frac{p^2}{meE} \qquad (13)$$

[10] *Phil. Mag.*, S-6, Vol. 23, pp. 86–94, 1912.

Now suppose that I should write down some differential equation other than that of classical dynamics, and solve for the relation between the radius of the orbit and the quantity p which appeared as the angular momentum in the classical theory. I should obtain

$$a = f(p) \qquad (14)$$

where $f(p)$ is different from the right-hand side of (13), so that, in general, equations (13) and (14) would not be consistent. There might be certain values of p for which they were consistent, however, and if we take the classical law plus this new law as the expression of the laws of motion, they will together restrict the motion to these orbits. Thus suppose we consider the equation

$$\left(p - \frac{h}{2\pi}\right)\left(p - \frac{2h}{2\pi}\right) \cdots \left(p - \frac{nh}{2\pi}\right) = 0 \qquad (15)$$

expand the left-hand side and, having picked out the term in the first power of p, equate p to the remainder of the expression, or rather its negative, say $\varphi(p)$, to which this equation makes p equal.

Suppose now we take as our dynamical laws the classical dynamical laws, whose solution is expressed in

$$a = \frac{p^2}{meE}$$

and the additional equation

$$a = \frac{[\varphi(p)]^2}{meE} \qquad (16)$$

These equations will only be consistent for the values of p given by

$$p = \frac{h}{2\pi}, \frac{2h}{2\pi}, \cdots \frac{nh}{2\pi}$$

which make p equal to $\varphi(p)$.

Thus if classical dynamics and equation (16) (which is itself a differential equation) be taken together as constituting the laws of dynamics, they will, for circular orbits, restrict the motion to the various orbits corresponding to the angular momenta

$$\frac{h}{2\pi}, \frac{2h}{2\pi} \cdots \frac{nh}{2\pi}$$

By taking $\varphi(p) = p + A \sin\frac{2\pi^2 p}{h}$

where A is a constant, we obtain an expression which gives $\varphi(p) = p$ for $p = \frac{nh}{2\pi}$ where n has all values up to infinity. Of course, in a sense, the equations $\int pdq = nh$ provided by the Wilson-Sommerfeld theory constitute supplementary conditions analogous to equation (16), but the advantage of the latter is that it is in the form of a differential equation. It will of course be understood that the foregoing is only intended as a brief sketch illustrating the general idea of the process. It is a process in which

the mind has to accustom itself to accept as fundamental a more restricting relation between the function of the position of the electron and the various time derivatives of its motion than can be expressed in a single vectorial differential equation. It must remain incomplete as does the restriction involving $\int pdq$ until formulated in such a way as to permit of the departure from one stationary state and the entry into another as determined by those phenomena, emission of unifrequentic radiation, emission of quanta, etc., which we associate with the transition.

THE DESCRIPTION OF MOTION IN THE CASE OF NON-LINEAR EQUATIONS

An interesting situation would have arisen had the circuital relations of electromagnetic theory been non-linear. It would then have been impossible to have added two solutions and obtained as a result a field which was itself a solution. The story of two moving singularities would have to be sought as a solution of the field equations of such a nature that it contained two singularities, and even the most general solution would not permit of any motions we chose to assign. The field equations themselves would have a power to restrict the motion of one singularity in relation to another, a power which is denied to the electromagnetic field equations on account of their linearity, a linearity which thus gives mathematical simplicity at the expense of content, and necessitates the addition of a force equation to make up for the deficiency.

In a sense, the determination of the interactions between electrons by regarding them as singularities in the solution of non-linear equations is philosophically very satisfying. The process consists in setting up differential equations in which the quantities talked about are singularities of the solutions, and then seeking a one-one correspondence between these singularities and the observable entities of nature. The whole story is told in one set of equations. The reason for the existence of stationary states of quantum theory would then find itself in the fact that there would be no solutions of the equations corresponding to motions of the singularities other than the motions desired. The transition to classical theory would find its way by virtue of the ordinary force equation of electrodynamics being a close approximation to the law of the singularities for the cases where it was applicable. Tempting as the possibilities seem from the philosophical standpoint, it would appear that the mathematical difficulties incidental to handling the situation would be very great. However, these difficulties are apt to demand our attention whether we like it or not. Our natural inclination is to describe the relative motions of particles by force equations, or at any rate by attempting to write a differential equation which describes the motion of one particle considered as a thing by itself in the field of the others. If, however, the nature of the complete theory is such that the particles form singularities in a set of non-linear equations, it is necessary to ascertain whether any additional law which may be imposed is consistent with the restrictions on the motion imposed by this condition.

I place some emphasis on this matter because it appears to be very pertinent in connection with the description of motion as given by Einstein's General Theory of Relativity. Much as I should like to speak at some length of this great revolution of our method of looking upon nature which has engaged so much of our attention during recent years, it is of course impossible adequately to deal with it in the few minutes I can devote to it. If, however, you will pardon the use of statements in a somewhat elastic form, I should like to sketch a few of the elements involved particularly in relation to the point I am discussing at present, the modes which we adopt for the description of motion.

GENERAL RELATIVITY AND THE DESCRIPTION OF MOTION; THE GENERAL INVARIANCE OF LAWS

The mind has decided that it likes the idea of a body moving in a straight line with constant velocity, and it would like to see everything going in that way. The sense in which we use the term straight line is that of a linear relation between the coordinates of space and time.

We can avoid the twofold ideas—straight line, constant velocity, by saying that we should like the body to move so that $\int ds$ is a minimum, *i.e.*, $\delta \int ds = 0$ where[11]

[11] I have avoided the introduction of the velocity of light, c, in the line element at this stage owing to a preference for having it make its first appearance as a constant in the expression for the line element which has to be chosen in order to obtain a law of motion approximating to the Newtonian law and with g's satisfying $G_{\mu\nu} = 0$. In this capacity c serves to determine a limiting velocity for planetary motion, and the choice of c necessary to explain the anomalies in the perihelion motion of Mercury is consistent with its being equal to the velocity of light. The nature of the occurrence of c in the line element is such that far removed from matter we have $ds^2 = dx^2 + dy^2 + dz^2 - c^2dt^2$. It is now a matter of mere algebra to show that this expression is invariant under the Lorentzian transformation, although it must be remarked that this does not imply the restricted theory of relativity in its entirety as a necessary consequence, for there is nothing to show that observers moving relatively to each other with constant rectilinear velocity will automatically adopt the relation of measures given by the Lorentzian transformation.

$$ds^2 = dx^2 + dy^2 + dz^2 + dt^2 \qquad (17)$$

Now if we imagine any arbitrary point transformation of coordinates,

$$x = f_1(x_1,y_1,z_1,t_1),\ y = f_2(x_1,y_1,z_1,t_1),$$
$$z = f_3(x_1,y_1,z_1,t_1),\ t = f_4(x_1,y_1,z_1,t_1)$$

the most general value assumed by ds is

$$ds^2 = g_{11}\,dx_1^2 + g_{12}\,dx_1dy_1 + \ .\ .\ .$$

where the g's are functions of the new coordinates.

A path which was a straight line in the old set of coordinates will not be a straight line in the new set, although both paths would be recognized in their respective coordinates as the paths which made $\int sp$ a minimum. The question then arises as to whether any arbitrary motion which might be assigned could be described as a straight line with constant velocity in some other system of coordinates. The answer to the question is No. In spite of the infinite number of different kinds of motion describable in this way, all can not be so described. There is something peculiar about all the g's which have come from the g's 1, 1, 1, 1, of (17) by transformation. That something can be expressed in necessary and sufficient form as a set of differential equations for the g's with the coordinates as variables. This set of differential equations has in it nothing to indicate what particular set of coordinates is used. If the answer to our question of whether all paths could be reduced to rectilinear constant velocity paths by suitable transformation had been Yes, the whole law of motion under any circumstances whatever could have been expressed in astonishingly elegant form in the statement that

A particle moves so that, with zero variations at the limits,

$$\delta \int (g_{11}\,dx_1^2 + g_{12}\,dx_1dy_1\ .\ .\ .\ )^{\frac{1}{2}} = 0$$

where the g's are solutions of the set of differential equations to which I have referred, particular solutions depending on the coordinates used, each solution being determined in its own set of coordinates by specification of the appropriate initial conditions. The general law would not involve the specification of any particular set of coordinates, and would therefore be of a form to satisfy the wish, which many people feel, to be able to express nature's laws in a form invariant under any transformation of coordinates. If, as I have said to be the case, such a law is not wide enough to cover the circumstances of nature, what then? Well, it may be that the laws of nature are expressed under

$$\delta \int (g_{11}\,dx_1^2 + g_{12}\,dx_1dy_1 + \ .\ .\ .\ )^{\frac{1}{2}} = 0$$

where the g's are solutions of some set of differential equations other than those which I referred to, a set of differential equations which is satisfied when the other set is satisfied, since the cases comprised under that heading must constitute a special case. The law in this widened form will still be the same in all systems of coordinates. Now what Einstein did was to suggest an appropriate differential equation for the g's such that the law

$$\delta \int (g_{11}\,dx_1^2 + g_{12}\,dx_1dy_1\ .\ .\ .\ )^{\frac{1}{2}} = 0$$

with the g's solutions of that set of equations, comprised the motions of astronomy, the understanding being that matter was to be regarded as existing at the places where the equations for the g's possessed singularities. All garnishing of this question by discussions of non-euclidean geometry of space is unnecessary for the statement of the law, and constitutes an elaboration clarifying to some minds and confusing to others, because the said others have formed about ordinary geometry intuitions which are stronger than they had any right to form. For here, as in many cases, the secret of understanding a subject which is abstract is a more clear realization of the fact that we ought not to have thought we understood the part which we regarded as non-abstract.

Now the main reason why I have dragged in this discussion of the Einstein theory is this. The motion of the particle has been described by the geodesic law, and the problem of the motion of one particle about another particle, regarded as fixed, was of course solved by Einstein. The g's which occur in that calculation are the g's as calculated for the fixed particle alone, and indeed the process of finding them was to find a solution of the differential equations which was symmetrical about the point which the particle was to occupy. Now, in electromagnetic theory, the linearity of the equations makes it possible to define what we mean by the field of one electron as distinct from that of another. But, the equations of the Einstein theory are non-linear, and this circumstance renders such a definition impracticable. We might still impart definiteness of statement into the geodesic law were we to say that the particle B moves in a geodesic in the field which the particle A would produce if alone in space. The non-linearity of the equations will not let us escape from the difficulty so easily, however, for, as I have already remarked, that very non-linearity itself insures a relation between the motions of the singularities from the mere fact that they are singularities in a solution of non-linear equations. Neither can we avoid the difficulty by diminishing very greatly the strength of the singularity whose motion we are discussing, with the idea of thereby diminishing its contribution to the g's; for, the *definiteness* with which the

motion of a singularity is determined by the non-linearity of the equations does not necessarily diminish with the strength of the singularity. The question then remains as to whether the law of the geodesic is less stringent, more stringent or the analytical equivalent of the law imposed by the non-linearity. Until this matter is cleared up, there is a danger that the law of the geodesic may say too much.

At first sight, it may appear that the difficulty may be avoided by that line of attack in which the theory is thrown into a form in which the Matter-Energy Tensor is used to define the presence of a matter, and a set of equations of motion are apparently built up without the use of the geodesic law, to which, however, an attempt is made to show them equivalent in a special case. A careful examination of the procedure will, I think, reveal however that, in the case of the motion of a body for example, the quantity which figures in the theory as a velocity is not necessarily the space-time velocity of the region of discontinuity between what is defined as matter and what is defined as empty space, and it remains as an additional postulate that solutions of the equations which Einstein uses for his g's can be found for which the two are consistent.

The sort of difficulty here discussed must arise in any scheme in which matter is defined as present at points where there exist singularities of solutions of non-linear systems of differential equations, and in which an attempt is made to describe the motion other than through the laws which the differential equations themselves impose on that motion.

A good deal might be said as to the causes which have been responsible for the desire to express nature's laws in a form invariant under any transformation of coordinates, and for this I have no time. I should like, however, to make one remark on the meaning of the invariance of physical laws. For simplicity, let me for the moment confine my remarks to invariance in the sense of the restricted theory of relativity.

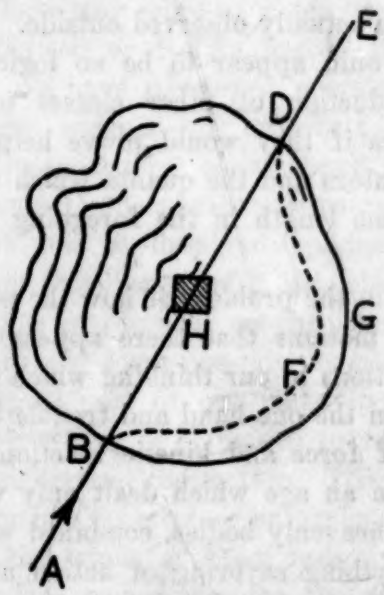
Suppose that we modify the equations of the Newtonian theory into a form consistent with the restricted theory, as in fact Poincaré did. Of course they will differ but very slightly from the Newtonian equations. Suppose the planets actually follow such a law. A particular solution of the equations will still correspond to one body, say the sun, with all the other planets moving in exact circles around it, apart, of course, from the mutual influence of the planets. Suppose I then made careful measurements and actually give θ and r as functions of t. The former would of course be proportional to t and the latter would be a constant, its magnitude being determined by the constant value of $\frac{d\theta}{dt}$. Suppose, now, I assert that the equations I have written down represent a law of nature since they are true for every planet I observe. Will my law be invariant under the transformation of the restricted theory? Certainly not. Is it inconsistent with the restricted theory? Certainly not. In fact, we started out with the supposition that the law I had found experimentally satisfies equations of motion consistent with the restricted theory, although I was not supposed to know that. Then here is a dilemma. The thing is true because I observed it. It is even a solution of equations consistent with relativity, but I must not call it a law because it itself is not invariant under the restricted theory. When is a thing which is law to be called a law and when is it not to be called a law?

If I should take my results concerning the planets' motions to a relativist he will say: "No, that is not the law; you have said too much. You have given an integration of the differential equations which express the law, and that contains much that is peculiar to your coordinates. The thing is not as invariant as all that." I shall ask him how much less I am supposed to say before he will permit my statement to pass as a law. He will show me those differential equations of the second order which, as we stated at the beginning, were supposed to be invariant, and of which my empirically found result constituted a solution. The relativist will say: "Now these differential equations say less than you have said, because, on integrating them there will be certain arbitrary constants which the equations themselves will not determine. These constants will be different for different systems of coordinates, so that what you will write down as the solution of these equations for that particular set of planetary motions which you have described will not appear the same in other sets of coordinates. Of course the most general solution of these equations, with the constants left arbitrary, will be of the same form in all systems of coordinates." Since, in order to say something which is invariant, I have been condemned to speak in differential equations of order no lower than the second, I now begin to wonder why I have been allowed even that amount of liberty. For I might have said less still by speaking in equations of higher order, since, in solving these equations it would have been necessary to specify more constants in order to make my solution definite. With equations of very high order, it would be necessary to add almost the whole story by the insertion of the various constants (and, in the case of partial differential equations, arbitrary functions),

necessary to proceed from the differential equations to the solution.

Thus, if any one should ask me to state the general spirit of the statement of the general theory of relativity, I do not think that I should say that it implies that the laws of nature are invariant under any transformation of coordinates, for that is not fair to statements of truths which would appear to have every right to be called laws, but would not be invariant. I should feel inclined to say that the spirit of the principle is that it is possible to say *something* in a form invariant under any transformation of coordinates, even though, in the limit, when the equations are of very high order, that something might amount to nothing. An important feature of Einstein's theory is that it speaks in terms of equations of only the second order.

Apart from its relativistic bearing, the most revolutionary aspect of Einstein's gravitational theory is its substitution in place of force acting in opposition to kinetic reaction as a starting point in our thinking, the properties of a mathematical curve as a more fit thing in which to find a representation of nature's laws. It is not a question of one method being right and the other wrong, but a clear realization that neither has any claim to ultimate fundamentality other than that which its simplicity implies. A very crude analogy will serve to illustrate the nature of the situation.



Suppose that the figure represents a crater with a house *H* in the middle, and that a traveler sets out to go from *A* to *E* by the shortest path. He will not necessarily pursue the path *ABHDE* leading down to the bottom of the crater, and through the house, because that may be too long. Nor will he necessarily go by the path *ABGDE*, because that may be too long. By taking some such course as *ABFDE* crossing the crater part of the way down, it is possible that he will find a path shorter than any other, and this is the path he will take. Suppose now that while this is true, we know nothing about it, and that we find ourselves seated high up in an airplane watching the spectacle. Of course I shall not see the crater as such, everything will appear flat. I shall see the traveler going from *A* to *E*, and shall wonder why he does not go straight across and through the house. If I have been taught in my youth that a body moves in a straight line unless a force acts on it I shall conclude that the house repels him. Having come to this interesting conclusion, I shall ponder over the reason why the house repels him. Possibly I shall receive a sudden inspiration which will lead me to believe that the house contains a man, who is provided with a hose which he plays on the traveler, compelling him to keep away from his property. I may be able to describe the traveler's path very accurately in terms of the hose. Now if you are philosophically inclined, you may doubt the reality of this explanation, and ask me whether I am certain that there really is a hose in that house. Of course, if you press me hard enough, I shall find difficulty in proving that there is. If you worry me enough, however, I shall get very angry with you and call you an impracticable philosopher; but, feeling the need of saying something to your arguments, I may finally talk to you in this way: "I care not whether there is or is not a hose in that house. So long as by picturing one there, and expressing the traveler's motion in terms of its influence I can predict results which are true, I am on perfectly firm ground. I am responsible to no man for how I think so long as my conclusions are correct." Having said this, I shall feel that I have justified my attitude, and confounded you completely. I shall begin to feel a bit of a philosopher myself; but I shall continue to enjoy secretly the picture of this hose and the various details of its action. I shall think all sorts of things about it which I shall never dare to tell you lest you should laugh at me. I shall wonder what the density of the liquid composing the hose stream may be, what its boiling point may be, and so forth, and I shall thank heaven that I am not like you who have no hose to aid you; for I shall wonder how your mind can think at all unless it has something to think about. Now suppose that while I am doing all this you should bring me some observations which show that the motion of the traveler was not exactly what we thought it was. The difference might be very slight, but it might be of such a nature as to upset completely the simplicity of the action which I had imagined as going on by virtue of the hose. Of course, I shall first cling to the hose, but shall modify it slightly. I shall say "Naturally this is no

ordinary kind of a hose. Possibly it does not push entirely in the direction in which the stream of liquid travels," but I shall have to face the situation that while the discrepancies may be small in amount they may be large in principle, and involve such radical alterations in my notions of the mechanism of the process that the hose which I shall have to picture will be radically different from any hose which I have ever seen. I shall go on in this way, modifying and adjusting the hose, making it more and more difficult to understand; and, forgetting that the original justification for its introduction was its apparent power to explain what was observed in terms of something which I thought I knew all about, I shall soon be in the position of expending 99 per cent. of my ingenuity in trying to understand the hose, leaving only one per cent. for the law of the traveler. Now suppose that, while I am doing this, and am feeling rather disheartened with my success, you should come to me and say, "I have made a discovery. I do not know why the traveler moves as he does, neither, I think, do you, but I have found exactly how he does move. He is moving from A to E by a path which is the shortest distance between those two points, not as the crow flies, but across a crater whose form I can describe to you in a very elegant way, and which appears to play a very important part in a lot of other phenomena going on down there, the direction which that little stream takes for example." Suppose you should say this, and should add: "Now, I am going to take this statement of the law as my starting point. If there is to be any hose in the matter, it is the hose which is going to be explained in terms of this fundamental law, and not the fundamental law in terms of the hose." I think I should have to admit that your attitude was at least reasonable. It is a change of viewpoint of this kind to which we have to adapt ourselves in passing over from the Newtonian description of motion to that adopted in Einstein's theory of gravitation.

CONCLUSION

In its fundamental aspects, practically the whole of modern physics is concerned with the discussion of the relations which exist between the motions of two sets of points in such a way as to establish a sort of one-one correspondence between the things which we do, and the behavior of one set of points on the one hand, and the things which we observe and the behavior of the other set of points on the other hand. We send a beam of electrons through an X-ray tube, and certain dark lines appear on photographic plates elsewhere, or electrons are emitted with certain velocities which we measure indirectly. As regards the electrons which are emitted into our apparatus, we can almost say that we observe them directly. As regards the blackening of the photographic plate, we are content if we can account for certain electronic emissions or motions to which we can attribute it. We do not, however, try to establish a direct relation between the original beam of electrons and the photographic plate or photoelectric cell, because we find that certain other apparatus was necessary for the experiment, a calcite grating and X-ray target, and so forth. These pieces of apparatus are replaced in the mind's eye by other sets of points grouped into atoms and molecules, in a manner characteristic of the substances in such a way that we may hope to be able to establish a relation between the first set of points, those in the target, those in the calcite and those in the final photographic plate or photoelectric cell. The whole problem is to discover how the points must be assigned and what function their mutual motions are of each other in order that the correlation may be satisfactorily made.

So far it has been possible to get along with points of two classes, the positive and negative electrons, and it would be considered artificial if we had to introduce anything else. The reason for this lies in the fact that only these two kinds of points occur in the specification of what we do and what we observe. In a certain sense we have isolated them, and the desire is to introduce into the atom nothing other than what we have actually observed outside. Apart from this, there would appear to be no logical objection to the introduction of other classes of points as intermediaries if they would prove helpful. Bohr's virtual oscillators and the quanta which we have discussed at some length in the foregoing are cases in point.

Now, it is in the problem of how the points control each other's motions that there appears that deference to traditions in our thinking which is the source of comfort on the one hand and trouble on the other. Our ideas of force and kinetic reactions which had their birth in an age which dealt only with the motions of the heavenly bodies, combined with a repugnance to anything savoring of action at a distance, has led us to describe the motions of any one of these points in terms of fields produced by the others. This description of the motion has invited many other ideas. We have felt the desire to define in terms of this field something whose radiation from one of the points is synonymous with decrease in the velocity of that point and whose influx into a region containing another point is synonymous with increase of motion of the point there in such a way as to maintain what we speak of as conservation of energy. The necessity of providing for the smooth working

of these ideas has suggested extending the points into finite sizes, with the attachments of numbers (the volume density) to the volume elements to which they are extended, in such a manner as to retain the total charge constant. It is in the mechanism which we have provided for interrelating the motions of the points that most of our difficulties have occurred. The mere description of that interrelation is not difficult, and could readily be written down. Such a description as a complete statement would, however, be regarded as artificial without an interlocking mechanism built up in accordance with the ideas which we have inherited from our studies of matter in bulk. It is only little by little that we will allow these modes of expression to depart from us. Each step must be allowed time to grow; and not until it is firmly entrenched are we willing to admit a further extension. Classical electrodynamics itself affords a good illustration of a stage in such developments. I suppose it would be fair to say that, to-day, the average physicist would be very content if everything could be correlated on the basis of classical electrodynamics. Yet, at the time when it was born, electrodynamics was as incomprehensible to most people as the quantum theory is to-day. The desire to interpret it in terms of the properties of ordinary matter was compelling, and afforded difficulties of primary moment. To-day we would be satisfied with the theory itself if it were complete, and would be glad of it, not because we have any more of a picture in the sense in which Maxwell or Kelvin looked for a picture, but because we have developed a state of mind in which we have widened our criteria for reasonableness, the change having come about through continual contact with the phenomena correlated by that theory which was at first incomprehensible. It is only by close familiarity with the phenomena that radical changes in physical assumptions, which they suggest, and the corresponding moulding of our thoughts become part of our make-up in the sense that when something is explained in terms of them we say we understand the matter. This development of mental attitude takes place naturally with greatest intensity in the minds of those radicals in physical thought who are responsible for the new points of view, so that it is quite natural to find, as we do find, that radicals in physics are apt to become extremely conservative in their own radicalism.

It is a point which has been mooted before that there may be many ways of understanding the universe in terms of different fundamental starting points; and, once one has become imbued with this view, he is not averse to adopting different methods for the discussion of different classes of phenomena; for it is not unnatural to find that correlation in a limited field of nature's laws may be made in terms which are simpler than those which would aim to correlate all the phenomena of the universe. I sometimes picture the state of business of an optician whose activity is devoted to the manufacture of lenses, but, hearing of the unsatisfactory state of the quantum theory of dispersion, shuts up his shop until the matter is settled or even attempts to use the theory when it is settled.

How frequently somebody puts forth a theory founded on concepts which seem to have no place in a complete logical scheme, which even imply a misunderstanding of the fundamentals and which the philosopher could knock into a cocked hat in a minute if he so chose. How frequently he uses these concepts in a manner which tempts the mathematical physicist to tell him that he does not know what he is doing. Yet, how aggravatingly frequently he deduces from the illogical mess some new consequence which is found to be right and which may even have no place in the more profound philosopher's scheme of things. There is something in the fact that if we write down any correlation of a group of n phenomena where n is large, the chances are that, no matter what we may be thinking about, or what our picture of the processes is, if that scheme of correlation predicts a new phenomenon, that phenomenon will be found in nature. Even in its full generality, the scheme of nature's laws is not so elusive and fickle but that if we fit some scheme of regularity to it at one place we may, with reasonable chances of success, extrapolate that scheme of regularity beyond the place where we made the fit and predict new results. And it would seem that there need be no fundamental objection to the process, provided that one does not trouble himself about non-essentials —provided that if he uses an electron with mass, energy and so forth, he does not wonder what its color is—provided that, if he uses an ether for the purpose of visualizing the propagation of effects in terms of elasticity and inertia, he does not trouble himself as to why it does not freeze out in space where it is so cold, and why he does not therefore get light reflected from the boundary, and why the ether does not boil as the stars pass through it.

A power of critical insight which will enable us to show that, in the last analysis, nothing is real and most things are meaningless is all very well in its way; but it will not always carry its owner very far, and may frequently lead him into pessimism. Perhaps the most hopeful condition is a combination of critical insight with a none too delicate conscience when smelling out the truth. The owner of this combination may be subject to the criticism of being a particularly vicious sinner since he knows better,

but his philosophy will save him the retribution of his misdeeds; for he will know not to concern himself with the color of the electron unless the matter has a bearing. He will be apt to reap only the wheat from his harvest and leave the tares.

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THE WHITE INDIANS OF THE SAN BLAS AND DARIEN

As a result of recent studies carried on in the Darien region of Panama, more specifically on the San Blas coast, certain facts concerning the reported White Indians have been brought to light.

The White Indians obviously express a form of albinism which has been termed imperfect or partial albinism by Geoffroy Saint Hilaire, Pearson and others. These terms signify that either the skin, hair or eyes, any two or all three may fail to express the full albinotic condition, but that one or more are, partially at least, relatively free from pigment.

Such a condition actually applies to the Indians in question. Many, if not all, of the White Indian males have frecklelike copper colored pigment spots of varying size, location and number, which evince an imperfect condition of albinism in the skin. Again the hair is not necessarily devoid of pigment, but in some cases shows traces of brown and in other cases is clearly auburn. Finally the iris is hazel (blue with brown spots), or dark blue, or dark violet. These observations clearly establish the fact that, if any classification of different degrees of albinism is valuable, as no doubt it is, the White Indians of Darien and the San Blas must be considered imperfect or partial albinos.

This contention, as opposed to the classification of these Indians as persons exhibiting idiopathic leucoderma, is supported by the fact that the condition obviously has a genetic basis, and that it is not the result of progressive development but is apparent at birth. Indeed its hereditary nature is demonstrated clearly in the hundred or more matings, the history of which I obtained during my residence in San Blas. If idiopathic leucoderma be considered as an inherited trait, then the terms imperfect and partial albinism and idiopathic leucoderma obviously approach a synonymous significance.

The origin of the condition expressed in the White Indians would seem to be most satisfactorily placed in the mutation theory. I saw no evidence of their origin being traceable to previous miscegenation with Caucasians.

The White Indians appear frequently from matings of brown San Blas Indians, resident on the islands just off the mainland, and on the mainland itself. Indeed this is the principal source of White Indians in the region, as whites are not permitted to mate with whites, and browns only very rarely mate with whites.

Do the White Indians form a race? If for the existence of a race one demands geographical segregation and permanency through the demonstrated production of likes by likes the White Indians can not be said to form a race, for they neither occur by themselves in segregated geographical areas nor are they permitted to reproduce their kind. But if in a definition of race one includes any group exhibiting strikingly differential characteristics which are insured of permanency by virtue of possessing a genetical basis, then the White Indians of the San Blas may be said to form at present a race which, due to artificial restrictions, is dependent for its continued appearance upon another race, carrying factors for the former's inherited differential characteristics in its germplasm. It is obvious that whatever definition is applied the difference is not qualitative but rather quantitative; the qualitative basis, genetical nature of differential characteristics, being present in either case. Whether or not it is admitted that the White Indians now form a distinct race there can be no question that, at least, they hold potentialities for race production.

Actually, it may be repeated, the whites do not occur by themselves in a segregated geographical area but are to be found in varying numbers in practically all the "brown" villages of the region, where they appear, from time to time, from matings of recessive carrying browns with recessive carrying browns. The whites form about 0.7 per cent. of the total population: an exceedingly high proportion for any form of albinism. This high proportion I attribute to the fact that intense inbreeding has occurred for some time in the Indian villages. Thus conditions are highly favorable for the frequent expression of any recessive traits occurring in the strain. Since all known forms of albinism are apparently recessive to the normal condition, and as albinism, like all recessive traits, occurs more frequently in consanguineous marriages than in the population at large, the observed high proportion of whites among the San Blas Indians is not astounding, in view of the constant inbreeding which occurs.

The exact method of inheritance functioning in the present instance is not yet established. The condition is apparently recessive to the normal, but it is not clear whether it is due primarily to the action of a single gene, in its interrelation to the whole chromosomal content, or whether the genetic composition

of the whites differs in more than one gene from that of the normal San Blas Indians.

A more detailed presentation of the observations made and the data collected will be made in the near future.

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THE JAMES SIMPSON–ROOSEVELT EXPEDITION OF THE FIELD MUSEUM OF NATURAL HISTORY

COLONEL THEODORE ROOSEVELT, JR., and his brother Kermit, sons of the former president, sailed from New York April 11 to lead an expedition through southern and central Asia under the auspices of the Field Museum of Natural History of Chicago. The magic of the name Roosevelt has led to much attention from the newspapers, and many questions are still being asked. To meet some of those which may arise in the minds of readers of SCIENCE, therefore, I have been requested by Mr. D. C. Davies, director of Field Museum, to make a brief statement regarding the expedition.

The possibility of such an expedition had been considered by Mr. Kermit Roosevelt for some time, but just what form it would take and whether or not his brother Theodore could join forces with him was not decided until quite recently. Like their father, both the younger Roosevelts have an extraordinary interest in natural history and a knowledge of animal life far beyond that of the average sportsman. They have read widely in the literature of zoological exploration and are by no means superficial in their grasp of fundamental problems connected with the study of the origin, evolution and distribution of faunas and special groups. As boys, they collected and prepared specimens to form small private collections of their own, and are thoroughly familiar with the detailed purposes and requirements of museum material. Therefore, like their father, they wished to have an expedition which would improve all opportunities to make it of lasting value to science. This was beyond their personal means, so they made known their desires to Mr. Stanley Field, president of the Field Museum. Mr. Field immediately took up the matter with Mr. James Simpson, a trustee of the museum, who had previously manifested his interest in the institution by the gift of its public lecture hall, known as the James Simpson Theater. Mr. Simpson, thereupon, agreed to provide the necessary support, involving no remuneration for the Roosevelts, but making it possible for them to carry all desired equipment for general zoological collecting and to employ trained assistants to cover special fields. The trip thus became a museum expedition and the principals have made their plans accordingly with a spirit quite as if they were regular members of the museum staff.

The expedition will enter India at Karachi and Bombay and proceed to Srinagar in Kashmir. Thence it is planned to cross the Himalayas *via* Leh and the Karakoram Pass, to make excursions into the Pamir region and then to push on across Turkestan to the Thian Shan Mountains. Subsequent movements will depend upon circumstances, but it is hoped to work in other parts of Turkestan and adjoining regions. The duration of the trip also is uncertain and, if necessary to accomplish the desired results, it will doubtless be prolonged beyond the present year. Newspaper accounts have, perhaps, borne rather heavily on the idea that the region to be visited is wholly unknown and quite unexplored. This of course is not justified, for although it is remote and difficult of access, sportsmen and travelers, mainly British and less famous than Marco Polo, have traversed much of it, and various books and maps have been published. Many parts of it, however, offer wholly new fields for the modern zoological collector and carefully made collections are particularly desirable at this time to supplement those being made somewhat farther north by Roy Chapman Andrews and the Third Asiatic Expedition in connection with which the American Museum and the Field Museum have a cooperative agreement. Aside from brief notes in various serials, the only study of any considerable part of the vertebrates of the region is found in the "Scientific Results of the Second Yarkand Expedition," published in 1879.

For practical reasons, the expedition is accompanied at the outset by only one trained zoological collector, Mr. George K. Cherrie, but others will be engaged in England or in India, and no pains or expense will be spared to make thorough collections, at least of all the vertebrates of the regions visited. Besides the usual equipment for collecting skins, skulls and skeletons of mammals and birds, portable tanks, preservative and paraphernalia are being carried to enable attention to be given to reptiles and amphibians or freshwater fishes. The party will be divided from time to time and special collectors will be sent into particular regions to work uninterruptedly along particular lines. Mr. Cherrie is known especially as an ornithologist, but is competent in all branches of field work and, by his long experience on many previous expeditions, is especially qualified for the general supervision of work of this kind. He was at one time connected with the Field Museum in its department of zoology and, later, he held a position with the Brooklyn Museum. In recent years he has been a free lance collector employed only at intervals, mainly by the American Museum of Natural History. His free-

dom from engagement at this time combined with his warm friendship with Kermit Roosevelt, gained during association on the famous "River of Doubt" expedition in South America, led to his immediate selection for the present Asiatic trip. Needless to say, hundreds of volunteers from all parts of the country, trained and untrained and of both sexes, have sent in urgent pleas for employment, but none could be accepted.

The Roosevelts, themselves, will doubtless devote much of their time to big game hunting, but practically all of it will be directed to obtaining selected animals of different ages and sexes to fulfill the requirements of large habitat groups for the Museum. Among the animals to be especially sought for this purpose are Marco Polo's Sheep (*Ovis poli*) which, on account of its very long and gracefully curved horns, is generally regarded as the finest of all wild sheep; the Thian Shan Ibex (*Capra sibirica*), a magnificent species with scimitar horns averaging nearly a foot longer than those of the European Ibex, and the Markhor (*Capra falconeri*), handsomest and most prized of all wild goats. These and other rare and interesting ruminants inhabit very high and mostly very rough country at altitudes of 16,000 feet or more, where a large part of the time will be spent. In central Turkestan beyond the Himalayas, however, there is lower ground and warmer climate, and here it is hoped to hunt for long-haired tigers. Specimens of these tigers have reached one European museum and two races have been characterized indicating distinctions from the Manchurian Tiger and the southern forms. No specimens are in American museums, and the large fauna which is doubtless associated with animals of this kind is very little known.

WILFRED H. OSGOOD
FIELD MUSEUM OF NATURAL HISTORY

SCIENTIFIC EVENTS

THE INTERNATIONAL CONGRESS OF FORESTRY

BY agreement between the International Institute of Agriculture and the Italian government a committee has been established for organizing a world's forestry congress to take place in Rome early in May, 1926. The headquarters of this committee, which is composed as follows, are at the International Institute of Agriculture in Rome: *President,* Professor Arrigo Serpieri, director of the Royal Higher Institute of Agriculture and Forestry at Florence, member of the Italian government; *Vice-presidents,* M. Anders Fjelstad, delegate of Norway at the International Institute of Agriculture, and Dr. Alessandro Stella, director-general of Forests and State Lands at the Italian Ministry of National Economy; *Secretary,* Signor Ariberto Merendi, Chief Inspector of Forests at the Ministry of National Economy; M. Deoclecio de Campos, delegate of Brazil at the International Institute of Agriculture; Signor Gian Francesco Guerrazzi, delegate of Italian Somaliland at the Institute, and Professor Asher Hobson, delegate of the United States of America at the Institute.

The congress will bring together experts in forestry and the timber and allied industries from all parts of the world. At the same time, in conjunction with the International Fair at Milan, there will be held an important exhibition of forest products and the machinery used in their conversion, which will enable visitors to examine the different products of the wood-manufacturing industries and the wood-working machines made in the various countries. Various excursions to the more typical forest lands in Italy, and possibly in other countries, will be arranged to follow the work of the congress.

WORK OF THE NORTHEASTERN FOREST EXPERIMENT STATION

ANNOUNCEMENT is made by the Northeastern Forest Experiment Station that its work will be expanded during the coming year to include investigations of insect pests and tree diseases in cooperation with the Bureau of Entomology and the Bureau of Plant Industry. The investigations of insect pests are being financed in part by contributions from several individuals, who desire to remain anonymous, for the control of the white pine weevil. H. J. MacAloney, of the New York State College of Forestry at Syracuse University, will be in general charge of the work. The sum of $2,500 for each of the two succeeding years has been made available for the study, which will be conducted in cooperation between the Northeastern Forest Experiment Station, the Bureau of Entomology, the Harvard Forest, the New York State Conservation Commission and other interested agencies. The white pine weevil, at the control of which the study is aimed, is an insect which annually causes much damage by destroying the terminal shoots of young pine trees. This results in decreased growth, and frequently in malformation of the trees attacked. The insect is distributed throughout the eastern United States and is one of the serious drawbacks to the profitable growing of white pine.

Other entomological studies which will be conducted next year at the Northeastern Forest Experiment Station include observations on the larch sawfly, which some forty or fifty years ago practically exterminated all the mature larch in the northeastern states, and which appears again to be becoming active in increasing numbers. This work will be handled by J. A. Beal, of the New York State College of For-

estry, who will also study the part played by insects in the disintegration and disappearance of logging slash and of the possible danger of breeding insects injurious to living trees in such slash. Cooperation between the Forest Experiment Station and the Maine Forest Service in studies of the spruce budworm and other destructive forest insects will be continued.

Dr. Perley Spaulding, a graduate of the University of Vermont and a member of the Office of Forest Pathology in the Bureau of Plant Industry, will be assigned to the Northeastern Forest Experiment Station for the studies of tree diseases. Dr. Spaulding's investigations will stress particularly the chestnut blight, which has destroyed immense quantities of chestnut trees in the northeastern states, and which is now present throughout practically the entire range of chestnut. His studies will be directed particularly to the location and propagation of all trees which are apparently more or less immune to or are able to recover from the effects of the blight. It is hoped that certain blight-resistant individuals may be found from which it will eventually be possible to reforest areas where the chestnut has been destroyed with trees that are able to withstand the disease. Chestnut has been so important a source of fence posts, telephone poles, railroad ties and tannin extract, as well as a general utility farm timber, that it is exceedingly important to make every effort to save it from complete extermination. Dr. Spaulding's activities will also include, so far as time permits, studies of the white pine blister rust and of the decay of logging slash of different kinds of trees and under different conditions.

SIGMA XI AND THE CALIFORNIA INSTITUTE OF TECHNOLOGY

A NOTABLE event in the history of science took place on April 2 with the installation of a Sigma Xi Chapter at the California Institute of Technology at Pasadena. The installation ceremony was conducted by the national president, Dr. F. K. Richtmyer, who came to California for this purpose. The charter membership includes forty-one members of the institute staff, twenty-three graduate students and eighteen members of the Mount Wilson Observatory staff. The following officers were elected for the new chapter:

R. A. Millikan, *President.*
A. A. Noyes, *Vice-president.*
W. N. Lacey, *Secretary.*
R. W. Sorensen, *Treasurer.*

Following the installation ceremonies a dinner was served to 250 alumni of Sigma Xi, and responses were made by J. C. Shedd, president of the Sigma Xi Alumni Association of Southern California; W. L. Hardin, past president of the Sigma Xi Alumni Association; Dr. D. C. Miller, president of the American Physical Society, and Dr. P. Debeye, of Zurich, Switzerland.

At the close of the dinner a notable address was given by Professor Richtmyer, before an appreciative audience, on the subject of "Some aspects of X-ray work." The lecture was illustrated with apparatus furnished through the courtesy of the General Electric Company.

JOHN C. SHEDD,
National Secretary, pro tem.

THIRD NATIONAL COLLOID SYMPOSIUM

THE provisional program of the Third National Colloid Symposium at the University of Minnesota, to be held on June 17, 18 and 19, is as follows. Dr. Harry N. Holmes is chairman of the symposium.

Molecular weight and solution: W. D. BANCROFT, Cornell University.

Some new aspects of the surface tension of colloidal solutions which have led to the determination of molecular dimensions: LECOMTE DU NOÜY, Rockefeller Institute for Medical Research.

The orientation and distribution of molecules: IRVING LANGMUIR, General Electric Company.

Photographic sensitivity: S. E. SHEPPARD, Eastman Kodak Company.

Adsorption: DR. FREUNDLICH.

Catalysis by Metallized Silica Gels: L. H. RYERSON, University of Minnesota.

Colloidal water and ice: HOWARD T. BARNES, McGill University.

Colloidal chemistry of rennet coagulation: L. S. PALMER, University of Minnesota.

The viscosity of protoplasm: L. V. HEILBRUNN, University of Michigan.

Antigenic properties of bacterial toxins neutralized by surface tension depressants: W. P. LARSON, R. S. EVANS, H. O. HALVERSON, University of Minnesota Medical School.

Physico-chemical studies on blood coagulation: I. N. KUGELMASS, Yale University Medical School.

The effect of anions on the colloidal and chemical properties of aluminum hydroxide: LEWIS B. MILLER, U. S. Public Health Service.

The nature of soil colloids: PHILIP L. GILE, U. S. Bureau of Soils, Washington, D. C.

The colloid chemistry of soils: E. TRUOG, University of Wisconsin.

Soil water: F. J. ALWAY, Division of Soils, University of Minnesota.

Colloids in geology: W. J. MEAD, University of Wisconsin.

Lithopones: C. A. MANN, University of Minnesota.

The plasticity problems of rubber: W. J. KELLY, Goodyear Rubber Co.

An experimental study of emulsification on the basis

of distribution of size of particles: ALFRED J. STAMM, University of Wisconsin.

The centrifugal method for determination of the distribution of size of particles in colloidal solutions: J. B. NICHOLS, University of Wisconsin.

The motion picture machine as an aid to colloid research demonstrations: E. O. KRAEMER, University of Wisconsin; W. J. KELLY, Goodyear Tire and Rubber Co.; W. G. FRANCE, Ohio State University.

TOWNSHEND STITH BRANDEGEE

TOWNSHEND STITH BRANDEGEE, honorary curator in the herbarium of the University of California, died in Berkeley, California, on April 7, 1925, in the eighty-third year of his age. Mr. Brandegee was born on February 16, 1843, in the village of Berlin, Connecticut. He was a veteran of the Civil War, a graduate of the Sheffield Scientific School of Yale University in 1870 (Ph.B.), by profession a civil engineer. He was assistant topographer on Hayden's survey of the territories, with work largely in southwest Colorado. He was also connected with the northern transcontinental survey, being situated in the Yakima Region in what was then Washington Territory. He came to California in the winter of 1886–87 to superintend the collection of rare or troublesome tree trunks for the Jesup collection at the American Museum of Natural History in New York City. He explored the islands off the coast of California and Mexico and made extensive journeys throughout the peninsula of Lower California. He was a resident member of the California Academy of Sciences in San Francisco and an active worker in that institution for many years. He later lived in San Diego, California, where his herbarium and garden were visited by all botanists making trips to that portion of the country. His wife, Katherine Layne Brandegee, herself a distinguished botanist, died in 1920. The collections of Mr. and Mrs. Brandegee were presented to the University of California in 1906 and since that time both Mr. and Mrs. Brandegee were closely associated with the work of that institution.

W. A. S.

SCIENTIFIC NOTES AND NEWS

THE Barnard Medal for Meritorious Service to Science, awarded by the trustees of Columbia University on nomination of the National Academy of Sciences, has been given to Dr. Niels Bohr, professor of physics at the University of Copenhagen, in recognition of his researches on the structure of atoms. Previous recipients of the Barnard Medal are: Lord Rayleigh and Professor William Ramsay, 1895; Professor Wilhelm Conrad Von Röntgen, 1900; Professor Henri Becquerel, 1905; Professor Ernest Rutherford, 1910; Professor William Henry Bragg and Professor Lawrence Bragg, 1915; Albert Einstein, 1920.

THE Elliott Cresson gold medal has been awarded to Francis Hodgkinson, chief engineer of the South Philadelphia works of the Westinghouse Electric & Manufacturing Company, who has been prominently identified with steam-turbine design and construction work during the past thirty-one years.

GIFFORD PINCHOT, governor of Pennsylvania and formerly forester of the U. S. Department of Agriculture, and George Bird Grinnell, editor of *Forest and Stream,* have been awarded Roosevelt medals for distinguished service by the Roosevelt Memorial Association.

DR. HERBERT S. JENNINGS, professor of zoology at the Johns Hopkins University, has been elected a corresponding member of the Russian Academy of Sciences.

DR. W. W. KEEN, of Philadelphia, and Professor Sanarelli, of Rome, have been elected honorary foreign members of the Royal Academy of Medicine of Belgium.

DR. GEORGE GRANT MACCURDY, of Yale University and the American School of Prehistoric Research, has been elected a correspondent of the Società Romana di Antropologia.

DR. CLARK WISSLER, curator-in-chief of the division of anthropology of the American Museum of Natural History, has been elected to honorary membership in the Michigan Academy of Science, Arts and Letters.

SIR WILLIAM BRAGG, Fullerian professor of chemistry at the Royal Institution, and director of the Davy-Faraday Research Laboratory, and Dr. F. G. Donnan, professor of inorganic and physical chemistry in the University of London, are among those on whom the senate of St. Andrews University has voted to confer honorary degrees.

THE University of Edinburgh will in July confer its doctorate of laws on Dr. A. S. Eddington, Plumian professor of astronomy and experimental philosophy in the University of Cambridge.

THE Paris Academy of Sciences has elected as corresponding member Sir John Russell, director of the Rothamsted Experiment Station in the section of agriculture; M. Luc Picart, of Bordeaux, in the section of astronomy, and M. René Koehler, of the faculty of sciences at Lyons, in the section of anatomy and zoology.

DR. J. E. ACKERT, professor of zoology at the Kansas State Agricultural College and parasitologist of the experiment station, was elected president of

the Kansas Academy of Science at the recent meeting in Manhattan.

DEDICATION exercises were held recently to celebrate the beginning of operation of the Champaign and Urbana (Ill.), sewage disposal plant. As part of the exercises a tablet was unveiled with the following inscription: "On this site, 1897, the Champaign septic tank was built; designed by Professor A. N. Talbot. It was among the first of its kind in this country. Dr. Edward Bartow, 1913–1916, carried on the first experiments in this country on the activated sludge process of sewage treatment."

DR. H. F. NACHTRIEB, since 1887 head of the department of animal biology at the University of Minnesota, has been appointed professor emeritus.

PROFESSOR JAMES H. BREASTED, of the Oriental Research Institute of the University of Chicago, who has spent the last three months at Luxor and Cairo, has returned to the United States.

DR. J. P. BAUMBERGER, assistant professor of physiology at Stanford University, is leaving for Europe on an eighteen months' sabbatical leave. He will attend the National Colloid Symposium at Minneapolis and will then proceed to Europe where he will spend nine months in Brussels on a C. R. B. Educational Foundation fellowship in physiology.

DR. C. R. ORTON, plant pathologist at the Pennsylvania State College, has been granted leave of absence for a year of research work with seed-borne parasites at the Boyce-Thompson Institute for Plant Research at Yonkers, N. Y.

DR. S. C. LIND has resigned as chief chemist of the Bureau of Mines to become associate director of the Fixed Nitrogen Research Laboratory of Washington, under the direction of Dr. F. G. Cottrell.

DR. GEORGE R. HILL, JR., professor of botany and dean of the school of agriculture, of the Utah Agricultural College, has resigned, effective July 1, to accept a position as director of the agricultural department of the American Smelting Refining and Mining Company with headquarters at Salt Lake City.

DR. ARTHUR C. STRACHAUER, for seven years professor and chief of the department of surgery in the University of Minnesota Medical School, has resigned to become director of the new George Chase Christian Cancer Institute at the university, which will be opened on July 1.

AT the seventh annual meeting of the American Society of Mammalogists held in Washington, from April 7 to 11, Dr. Hartley H. T. Jackson, of the U. S. Biological Survey, was elected editor of *The Journal of Mammalogy*, and Mr. A. Brazier Howell, of the U. S. National Museum, corresponding secretary. The other officers of the society were reelected.

DR. WILLIAM DE B. MACNIDER, professor of pharmacology at the University of North Carolina, served as physician-in-chief *pro tempore* at the Peter Bent Brigham Hospital, Boston, during the week beginning April 12.

MISS ALICE H. FARNSWORTH, assistant professor in the department of astronomy at Mount Holyoke College, has been granted leave of absence from the department for the year 1925–26, and has been appointed to an instructorship at the University of Chicago for that period. She will spend the year in research in the department of stellar photometry at the Yerkes Observatory.

DR. RHODA ERDMANN, professor of zoology and cytology at the University of Berlin, has founded a journal devoted to the cultivation of tissues outside of the organism. The publisher is Gustav Fischer of Jena. Articles can be published in German, English, French or Italian. The journal will be published irregularly, as material accumulates.

THE members of the British Physiological Society visited Holland, during the first week of April, holding their monthly meeting in the laboratory of Professor Willem Einthoven, at the University of Leyden. Professor Einthoven, who is a member of the society, presided over the meeting.

DR. WOLFGANG KOHLER, professor of psychology in the University of Berlin, has been appointed visiting lecturer on psychology at Harvard University for the first half of the academic year 1925 to 1926.

HOFRAT PROFESSOR DR. F. X. SCHAFFER, professor of geology at the University of Vienna and recently director of the Vienna Natural History Museum, is in residence at Pomona College for the second half of the present semester. Professor Schaffer will give the Joseph H. Johnson lectures and will discuss topics connected with the Mesozoic and Cenozoic of Europe.

DR. GEZA DOBY, visiting professor from Hungary, assigned to the botanical department of the Michigan Agricultural College, is to give an address on his work with plant enzymes before the botanical and biochemical groups at the University of Chicago on April 27, Iowa State College on April 29, the University of Wisconsin on May 5 and to a joint meeting of the Sigma Xi and the Nebraska Academy of Science at the University of Nebraska on May 7. Dr. Doby, professor of biochemistry in the economic faculty of the University of Budapest, and Professor Alexander Kotlan, of the Royal Veterinary College of Budapest, came to the Michigan Agricultural College, the latter

as "visiting professor" in the department of bacteriology under the auspices of the American-Hungarian Foundation whose headquarters are at East Lansing, Michigan. No exchange professor was sent to Hungary this college year, but in the coming college year Dr. H. J. Stafseth, of the department of bacteriology, goes to Budapest as the exchange professor from the Michigan Agricultural College. The visiting professor from Hungary has not yet been appointed. Plans are under way for extending the activities of the American-Hungarian Foundation to other institutions, in addition to the Michigan Agricultural College, and to include graduate students of high scholarship as well as professors. Before their return to Hungary this coming summer, Dr. Doby and Dr. Kotlan will be available to give addresses on their subjects at such institutions as would be interested.

DR. ROBERT A. MILLIKAN, director of the Norman Bridge Laboratory of the California Institute of Technology at Pasadena, gave on April 6, 7 and 8 the first lectures under the newly established Robert Lardin Fulton Lecture Foundation of the University of Nevada. The titles of the lectures were "The electron," "Light waves" and "Stripping the atom."

THE Mellon Lecture of the Society for Biologic Research of the school of medicine of the University of Pittsburgh will be given by Dr. Frank B. Mallory on April 30 on "Hemochromatosis and chronic poisoning with copper."

DR. CHAS. T. KNIPP, professor of experimental electricity at the University of Illinois, gave a lecture illustrated by experiments before the faculty and students of the Missouri School of Mines, Rolla, on April 16, on "The vacuum as an aid in research."

DR. MERRITTE W. IRELAND, surgeon-general, United States Army, will be the principal speaker at a dinner concluding the celebration of the one hundredth anniversary of the founding of Jefferson Medical College. The celebration will occur on May 29, immediately following the sessions of the American Medical Association at Atlantic City.

DR. WILLIAM ALLEN PUSEY, Chicago, president of the American Medical Association, will give the commencement address at the University of New Mexico in June.

A MEETING of the Sigma Xi Alumni Association at the University of Pittsburgh was held on Monday evening, April 20, when the following program was presented: "The travertine of Oklahoma," by Dr. W. H. Emig; "Chromosomes in the genus trillium," by Mr. J. L. Cartledge; "Basis of classification of plant societies in western Pennsylvania," by Dr. O. E. Jennings.

THE Massachusetts State Senate has unanimously rejected an adverse report of the Committee on Ways and Means and has passed to be engrossed a bill establishing Egg Rock Island near Nahant as a refuge for wild birds, to be known as the Henry Cabot Lodge Bird Sanctuary.

A COMMITTEE has been formed for the erection of a monument to the Italian physiologist, Angelo Mosso, in his native town of Chieri.

HARRY KING, former chief draftsman for the United States Geological Survey and the General Land Office and for many years professor of topography and cartography at the old Columbian College, now George Washington University, died on April 20, aged seventy-six years.

DR. WILLIAM EDGAR GEIL, the American explorer, lecturer and author, died in Venice on April 12 on his way home from Jerusalem where he was conducting research for the British Museum. Dr. Geil was in his sixtieth year.

THE annual meeting of the Royal Society of Canada will be held in Ottawa on May 19, 20 and 21. In Section V—Biological Sciences—the presidential address will be delivered by Professor Andrew Hunter, of the University of Toronto, his subject being "Proteolysis and the structure of proteins." The program includes fifty-eight titles grouped under three headings: Zoological, medical, physiological and biochemical, and botanical.

THE American Medical Association will hold its annual meeting at Atlantic City, beginning on May 25, some twenty other medical societies holding meetings before or after the main convention.

DR. EDWARD PLAUT, president of Lehn & Fink, Inc., has, in memory of his father, former president, appropriated a sum of money for the establishment of a research foundation, to be known as The Plaut Research Laboratory. This will be housed within the laboratories of Lehn & Fink, Inc., at Bloomfield, N. J. Commodious new laboratories have been provided, equipped in the most modern manner. The work to be undertaken will be a continuation of a series of researches which have been carried on by a group of men in the Research Laboratory of Lehn & Fink, Inc., for a number of years. The results of these researches will be published from time to time in the appropriate current journals. The staff comprises William H. Gesell, director; Moritz Dittmar, Ph.D., pharmacognosy; Emil Klarman, D.Sc., organic chemistry; Victor Ross, Ph.D., biological chemistry; John T. Stern, D.Sc., physical chemistry; John L. Ulrich, Ph.D., physiology; Mary Jacobsen, D.Sc., librarian. K. George Falk, Ph.D., and Herman Sharlit, M.D., have been retained as consultants.

THE French government is proposing to erect an observatory and wireless station on Mount Salève which towers 4,000 feet above Geneva. The observatory is to be installed with all modern instruments for meteorological and seismographic observation and research work. A telescope with a lens diameter of two and a half meters is now being constructed in the United States. Eventually it is hoped the station will become the seat of a meteorological office, and it is for that reason the wireless station proposed will be powerful enough to communicate with any part of the world.

THE trustees of the Polar Research branch of the Captain Scott memorial—the Lord Mayor, Sir Charles Sherrington, president of the Royal Society, and Lord Ronaldshay, president of the Royal Geographical Society—have decided to offer to hand over the balance of their fund, amounting to £13,000, to the University of Cambridge towards the erection, endowment and maintenance of a "Captain Scott Polar Research Institute." The institute, established in 1920, has so far been housed in the Sedgwick Museum, and has done good work in advising and assisting in connection with some smaller expeditions in the last four years. It has also, from grants made from the Scott Fund, been acquiring maps, sketches and publications connected with Polar research.

THE United States Civil Service Commission announces competitive examination for the position of chemical engineer to fill a vacancy in the Fixed Nitrogen Research Laboratory, Department of Agriculture, Washington, D. C. The entrance salary is $3,800 a year and advancement in pay may be made without change in assignment up to $5,000. Competitors will not be required to report for examination at any place, but will be rated on their education, training, experience, fitness and writings to be filed with the application, which should be filed before May 19.

TENTATIVE plans have been made for the organization of the American Refractories Institute, to provide a satisfactory contact between all the industries that use and manufacture refractories, in order to meet their various economic and technical problems with respect to heat-resisting materials and to maintain a research laboratory to study the problems of the consumer and the manufacturer. MacDonald C. Booze, senior incumbent of the Multiple Industrial Fellowship on Refractories at the Mellon Institute of Industrial Research, Pittsburgh, has been appointed temporary secretary of the organizing committee.

THE Norwich Castle Museum Committee of the City Corporation has under consideration, as we learn from *Nature*, the celebration of the centenary of the foundation of the museum under the presidency of the Norwich botanist, Sir James Edward Smith, F.R.S., in 1825. The history of the museum shows that its fortunes were of a varying character until 1894, when the corporation took over the collections of the Museum Society and housed them in the spacious galleries adjacent to the castle.

THE *Chemical Bulletin* states that a bill has been introduced in the Wisconsin legislature the purpose of which is to give scientific assistance to district attorneys in the investigation and solution of crimes. According to the terms of the bill power is to be given, under the provisions of the contemplated act, to the attorney general to appoint a group of scientific men who by education and experience are qualified to render impartial and valuable assistance in the investigation of crimes occurring in that state. Chemists, physicians, toxicologists, bacteriologists, pathologists, metallographers, finger-print and hand-writing experts, psychiatrists, and so forth, would be included in the list. No new commission is to be created nor are such experts to receive compensation from the state. The county calling for their services is to pay them a per diem fee as may be arranged in each case.

THE British Safety in Mines Research Board, in the course of an inquiry into the possibility of improving the present official tests of explosives intended for use in gassy or dusty coal mines in Britain, has arranged for seven typical British "permitted" explosives to be put to the official American tests, which differ in many respects from the British tests. The British explosives have passed the American tests, but the British "permitted" explosives were not so strong as the American when judged by the specified strength tests, nor so sensitive to detonation as judged by the explosion by influence test. This is attributed to the fact that the British explosives contain as a rule a far higher proportion of cooling salts.

UNIVERSITY AND EDUCATIONAL NOTES

THE University of Chicago has received a gift of $500,000 from the Wiebolt Foundation toward the development fund of $17,500,000, which is now being collected by the university.

THE New Jersey Legislature has appropriated two hundred thousand dollars for the erection of a new physics building for Rutgers College.

THE Iowa Child Welfare Research Station of the University of Iowa has received a grant of $133,500 from the Laura Spelman Rockefeller Memorial Fund to be used for research in the development of children.

DR. WILLIAM T. SANGER, secretary of the Virginia State Board of Education, has been elected president of the Medical College of Virginia in succession to Dr. Stuart McGuire.

DR. LESLIE B. AREY, recently appointed chairman of the division of anatomy at the Northwestern University Medical School, has been made the first incumbent of the newly established Robert Laughlin Rea professorship in anatomy. This chair was created through the gift of $100,00 by Mrs. Mollie Manlove Rea as a memorial to her husband, at one time professor of anatomy in the university.

DR. HARRY C. TRIMBLE, assistant professor of chemistry at the University of North Dakota, has resigned to become assistant professor of chemistry at the Harvard University Medical School.

DR. ALEXANDER ELLETT, National Research Fellow, who has been working at the Bureau of Standards, has been appointed assistant professor of physics at the University of Iowa.

DR. WILSON L. MISER, associate professor of mathematics, Armour Institute of Technology, has been appointed professor of mathematics at Vanderbilt University.

DISCUSSION AND CORRESPONDENCE

SERIES IN THE ARC SPECTRUM OF NITROGEN

IN the issue of SCIENCE for September 12, 1924, I presented some series regularities which I attributed to the quintet system of the spark spectrum of nitrogen. Quite recently Merton and Pilley (*Proc. Roy. Soc.*, A 107, p. 416, 1925) have published an excellent list of lines which they attribute to the *arc* spectrum of nitrogen. The occurrence of multiplets among these lines involving the same terms as occur in the infra-red multiplets has led to the conclusion that the infra-red lines are also members of the arc spectrum, and that the multiplets previously published belong to the quartet system and not to the quintet system. An analysis of Merton and Pilley's lines and additional infra-red lines measured at the Bureau of Standards has revealed the existence in the arc spectrum of nitrogen of doublet and quartet series systems consisting of combinations between P, P′, S, D and F, terms. In each system the lowest term so far found is a P term. Three PD multiplets of the quartet system have been found which can be represented very accurately by a Ritz formula, giving a value of 33992 to the low $^4P_3$ term. The details of the investigation will be presented in a forthcoming paper.

C. C. KIESS

THE UNDERTOW

THERE are many geologists grateful to Professor Davis[1] for calling attention to the rather careless acceptance of the undertow without adequate consideration of what it is and where it is. Everybody will agree with him that there is an undertow phenomenon produced in every wave trough, that the undertow becomes exceptionally concentrated and powerful off pocket beaches, that when the wind is blowing on shore the undertow effects are greatest, and that there is commonly confusion between undertow, littoral currents and tidal currents. However, it should be borne in mind that sea waves are of two kinds: deep sea waves which are oscillatory and which involve theoretically no translation of water longitudinally, and roll waves in which there is no oscillation and a maximum amount of translation of water. The reality of the undertow becomes evident when one considers the case of waves approaching the shore at time of low tide. For a distance upon shallow shores of half a mile there is a constantly renewed pouring shoreward of water where the oscillatory waves change into roll waves. This series of forward surging roll waves actually achieves a shoreward movement of water of prodigious quantity which returns to the sea only as undertow.

The very configuration of a shore indicates the reality of undertow. Material eroded and broken loose by wave action lies exposed at the shore, but although shingle forms well-marked beaches, it is not usually distributed upon the wave-cut terrace. In some manner the broken material is carried away from the beach, across the wave-cut terrace, and deposited in the wave-built terrace. Although it is certain that the undertow is notably concentrated at pocket beaches, nevertheless it is in existence elsewhere as wave-cut terraces and the wave-built terraces which skirt cliff bound shores past bay and headland alike clearly demonstrate. There seems little question that confusion between littoral currents, tidal currents and undertow is very difficult to avoid because they are all in existence at certain times in the same places.

Nevertheless, wherever waves break, irrespective of the direction of wind or shape of the shore line, there is a forward movement of water at the surface which can return only as an undertow; over wave-cut terraces where oscillatory waves are rare and where waves of translation are the rule the total amount of undertow return must be great and extensive in a direction offshore.

T. T. QUIRKE

UNIVERSITY OF ILLINOIS

[1] W. M. Davis, SCIENCE, Vol. LXI., February 20, 1925, pp. 206–208.

ORIGIN OF THE LOESS OF THE PALOUSE REGION, WASHINGTON

THROUGHOUT east-central Washington, especially that region known as the Palouse wheat country, is a large area underlain by basalt, which is a part of the Columbia lava plateau. Above this basalt is a covering of very fine sand, which has a thickness as great as 250 feet and more.

This fine sand has the characteristics of loess. When exposed in highway and railroad cuts, it stands with a vertical face, and in drying it shows vertical cleavage. It is composed of fine quartz sand, whose grains average 0.05 mm in diameter. The sand grains are sub-angular. The formation is iron stained and contains small concretions and tubelets of iron oxide. This loessial formation has two phases: one phase is massive, the other laminated, that is, stratified in thin, well-defined layers. These two phases are identical in composition, but differ in structure.

From a study of the field conditions, the writer has reached the following conclusions in regard to the origin and occurrence of the Palouse loess:

(1) That the loess was derived from the retreat of the continental ice sheet of pre-Spokane age, possibly Iowan or Illinoian.

(2) That the loess was deposited in shallow, sluggish, ephemeral sheets of glacial water to a depth of about 250 feet on the surface of the flat-lying basalt before the period of Pleistocene deformation.

(3) That a mature type of topography was developed on the surface of the loess.

(4) That a period of deformation followed, during which the region was elevated.

(5) That, following the uplift of the region, rejuvenation of the major streams came into evidence, developing deep canyons which were cut, first through the loess and then through the hard, underlying basalt.

(6) That later, recent action of the wind shifted the water-laid loess, modifying its laminated structure to the massive type, and causing surface features to be developed characteristic of aeolian deposition.

(7) That mud flows have subsequently developed on plowed hillsides during the rainy seasons of the year, modifying much of the surface covering so that the underlying laminated material has been largely lost to view.

Owing to the modification of the original loessial deposit by the surface action of wind and water, the underlying laminated phase is exposed only in widely separated localities, where deep road cuts have recently been made. This laminated phase is considered by the writer to be the original loessial deposit.

The writer proposes the name Palouse Formation to these sedimentary beds. The fertile soil of the Palouse region is largely a residual modification of the Palouse formation and has locally been redeposited by wind and surface wash.

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THE STEM OF MAGNOLIA AS A LABORATORY TYPE

THE general cellular structure of the woody, dicotyledonous stem is commonly illustrated in general botany courses by such forms as *Tilia, Aristolochia, Menispermum, Ricinus, Liriodendron,* etc. Each of these has certain limitations and disadvantages. To be most desirable for teaching vascular anatomy to elementary students, it would seem that a stem should have the following characteristics: (1) It should be generalized—not so highly specialized as many of the forms in common use; (2) its various groups of tissues should be easily recognizable; (3) the vascular bundles should be especially distinct and clearly delimited by broad medullary rays; (4) the annual rings should be well defined; (5) the stem should exhibit several different kinds of mechanical tissues; (6) the protoxylem should be persistent and easily distinguishable from the secondary xylem.

None of the stems ordinarily studied and figured in the text-books combine all these features, although some are very desirable in certain ways. In endeavoring to find a stem which might approach the ideal type described above, we have examined a great many forms, but none has given greater satisfaction than *Magnolia.* In the young twig the bundles stand out with diagrammatic clearness; they are equally as well defined as in the woody climbers usually studied. The protoxylem is as conspicuous as in *Ricinus.* The phloem areas are particularly well differentiated, a feature lacking in many stems. Sieve tubes and companion cells are as distinct as in the classic corn stalk. A few bast fibers occur among the conductive elements of the phloem, but most of them are localized in separate groups just outside of the phloem, as in *Aristolochia.* Stone cells and collenchyma occur in the cortex, the former showing very clearly the characteristic pitting. The epidermis is highly cutinized. Epidermal hairs occur on the young stem in abundance.

A notable disadvantage of *Magnolia* is that sections of older stems are cut with difficulty, owing to the hardness of the wood. Young twigs may be cut freehand with a fair degree of facility, but older ones must be handled by special treatment. We have used only *Magnolia grandiflora,* but suppose that other species would be equally as desirable.

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SCIENTIFIC BOOKS

Calculus of Variations. By GILBERT AMES BLISS. Published for the Mathematical Association of America by the Open Court Publishing Company, Chicago (1925), pp. xiii, 189. Price, $2.00.

WHEN it was announced some time ago that the first number of the Carus Mathematical Monographs was to be written by Professor Bliss on the subject to which he has made so many important contributions, it was felt by some that Professor Bliss had set himself a very difficult task. The main purpose of this series of monographs is the "diffusion of mathematics and formal thought as contributing to exact knowledge and clear thinking, not only for mathematicians and teachers of mathematics, but also for other scientists and the public at large," and it seemed that of all branches of mathematics the Calculus of Variations might easily prove one of the most difficult to present in a popular manner. This must have been, especially, the thought of those who were, like ourselves, introduced to the subject in a physical treatise expounding Hamilton's conception of the proper mode of statement of the laws governing the development of a dynamical system. We can still see in our mind's eye the formidable pages of δ's which seemed to differ in some mysterious way from the mannerly differentials of the preceding and following paragraphs; which had a ghostly superiority since they could never physically be realized; but yet which could be interchanged as to order of procedure with the more homely and familiar time differentiations of the subject. Could it be possible to present this subject in an elementary and straightforward manner, intelligible to the ordinary student of physics, with the usual familiarity with simple differentiation and integration?

There can be no doubt as to the answer in the mind of any one who reads Professor Bliss's book. The exposition is masterly and is as lucid as the best French mathematical treatises. Beginning with an interesting historical introduction, typical problems of the Calculus of Variations are described, such as, for examples, the shortest distance problem, the brachistochrone problem and the problem of the surfaces of revolution of minimum area (the soap film problem). Before taking up the general theory the author adopts the pedagogically sound procedure of treating these special problems in detail and he is able, without ever abandoning the "elementary" viewpoint, to illustrate in these problems the general modes of solution of problems in the Calculus of Variations and to point out the kind of difficulties that may be expected to arise. The presentation is very "modern" and the consistent use of Weierstrass's ingenious concept of a "field of extremals" and of Hilbert's invariant integral makes the theory straightforward and easy.

Jacobi's interesting theorem as to conjugate points here presents no difficulty. The final chapter, headed "A more general theory," is easy reading after the previous chapters dealing with special problems have been mastered and the book ends with some interesting historical remarks and a well-considered and useful list of bibliographical and historical references.

On the whole we think this book an illustration of the truth of Benjamin Franklin's dictum that no theory is difficult if properly and methodically presented. No student of physics of chemistry needs to be told of the importance of the subject-matter, for most physical theories seem incomplete until they can set up a Hamiltonian integral dominating the subject by the fact that its variation must be zero. Or, to give an illustration which will be more appealing to those interested in quantum theory, it may be said that any conservative dynamical system in which the coordinates are q and the momenta p lives in such a way that its life line in its representative (q, p) space is an extremal of the integral $\int \Sigma\, p\, dq$ where all comparison curves satisfy the equation $H = \text{constant}$ (H being the total energy of the system). We heartily recommend Professor Bliss's book either as a text for a short course on the Calculus of Variations or to any serious scientist for private reading. May the succeeding monographs of the series maintain the standard set by this first one!

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THE NORWEGIAN IDENTITY BOOK

AT the International Emigration Congress, held in Rome from May 15 to June 6, 1924, a proposition was introduced by the Norwegian delegate Fjelstad which, if feasible and viable, may go far toward solving the vexed question of the identification of the individual in intranational and international relations. It was devised by Drs. Jon Alfred Mjoen and Jon Bö at the Winderen Laboratory, Christiania, and proposes, in brief, the biological registration of a given population by means of an identity book,[1] so arranged as to include and, for public purposes, replace the ordinary certificates of birth, baptism, vaccination, school attendance, university matriculation and graduation, marriage, tax-payments, life insurance policies, military, naval, police court or prison relations and other personal papers establishing the true status of the individual in society. Its object is to facilitate and obviate bookkeeping in passport offices, courts of justice, at the ballot box or in any other situation

[1] J. A. Mjoen and J. Bö: *Eugenics Rev.*, Lond., 1924, XVI, 183–188.

in which the identity of the individual as a reliable or dubious social unit becomes of paramount importance. But the very object of this Scandinavian invention is to protect Abel against Cain, to see to it that the decent and honorable (the mass of mankind) do not suffer unduly for the "crimes, whims, ineptitudes and insanities of the few," to prevent the miserable, unwarranted espionage and hold-up policies which have harassed and tormented the peoples of Europe and America ever since the beginning of the World War. As to the other side of the shield, our Scandinavian authors speak in no uncertain language:

In all the belligerent lands, there were actually regulations which prevented criminals from being sent to death. It is equally characteristic of the organization of our modern society that the law-abiding are restricted in the exercise of their right of self-determination, because there exists a growing minority of law-breakers. In the human ant-hill there are none to exterminate the parasites. On the contrary, these parasites flourish and strut around as they have never done before and make their way with well-filled valises through all entrance gates, from the Riviera to Gjedser-Warnemunde, from Ukraine to Castle Garden. They manage to cross frontiers and pass through one land to another without standing in a queue on Alexander-platz, whilst the unpracticed, respectable citizen of another country must run from consulate to consulate, from one police station to another, to be cross-examined, suspected, nay, even thrown into prison, until, wearied out and discouraged, he gives up all further attempts to get across the border. The drunkards make it necessary to fashion regulations which fall heavily upon the temperate, the international criminal necessitates a system of passports, of control, and finally of punishment, which becomes more and more intolerable for the industrious members of society. The fault lies mainly in the fact that in the passport office, or in the sick insurance, at the ballot box or in the court of justice, no sincere effort has hitherto been made to identify a human individual or to connect the individual with his own past. The basic principle for the modern treatment of the individual at all the cross-roads of life is this: "Because there are to be found individuals with a vicious past, we must also deal with you as if you were equally vicious." The claim that every human being has a right to make, namely, the claim to be held responsible for his own acts, for his own motives and for his own way of life, is rendered difficult by the fact that we have hitherto had no means of identifying the individual. We have lacked a system which could promptly and in a practical manner establish the fact that "I am I, and nobody else." It is not very reassuring to see the powerlessness which is displayed by the so-called authorities of the state, even in the leading civilized lands, when it is a question of ascertaining the material composing the population and its movements, or when law and order need to be maintained and social evils combated. Milliards of money are granted annually for education, public health and the prison system, with the result that hospitals, lunatic asylums and prisons must be built larger and larger. And the prisons are filled, although most of the criminals still remain at liberty.

Our national and international conditions afford in reality to the criminal the fullest opportunities of constantly extending his fields of operation. Even in those cases where anti-social activities do not display criminal methodicalness and foresight, we see that these anti-social race-elements inflict great damage on the community. For they infect society biologically and morally. The biologically inferior—the mentally deficient—fill the ranks of the chronic drunkards, castaways and loafers, and may spread venereal disease. With the increasing movements of the populations which are now taking place these race-elements are being dispersed more than ever over new fields of operation.

In our human age it is becoming—and rightly so—constantly more and more difficult for society to rid itself of the inferior individuals by brutal methods. Formerly recourse was had to capital punishment. The method was radical and killed all evil growths at the root. But in our modern society, with its increasing feelings of mercy, the difficulties augment themselves with every new generation. The upright and industrious individual is often placed under control as if he or she belonged to the class of drunkards, prostitutes or criminals, especially if they remove themselves some distance from their native place. The demand for a new solution of these problems is making itself more and more clearly heard.

That such a plan would defeat its own object if controlled by unscrupulous political henchmen of jack-in-office type is self-evident. It is also obvious that the loss of his identity book would put the individual in a tight place, in the absence of his personal papers, and that the book itself would have to be safeguarded against falsification. But it is urged that the immense amount of time consumed and of energy dissipated in identification by ordinary methods has become so costly to government (for in modern life time is money) that the identity book will not only protect and defend citizens who have been slandered, rendered suspect, falsely accused or wrongfully arrested, but will also constitute a check upon the habitual criminal and recidivist. At the same time, a given book, if lost, will be at least valueless to the finder. In virtue of the identity book, the complement of an emigrant ship can be easily filtered, more people will vote at the polls when the troublesome formalities are dispensed with, the character of servants and other people seeking employment will be easily ascertained and registration of taxpayers facilitated, as also military mobilization, the prevention of smuggling, the apprehension of spies, the relief of deserving poor and of distressed sailors, while the passport system at frontiers will be enormously simplified. On the death of the holder, it is proposed that the identity book be returned to the Biological Registration Office as a public record. In view of its important bearings upon emigration and immigration, the proposition was

printed *in extenso,* in English, French and Italian, and referred to a special conference. The conclusions of the conference will be awaited with interest.

SPECIAL ARTICLES

A HIGHLY SENSITIVE PHYSICAL METHOD FOR DETECTING PROTEINS IN A SOLUTION

IT is generally believed that immunological reactions are the most sensitive, as they make it possible to detect such minute amounts as 0.00000005 grams of proteins (1/20000000). H. G. Wells, in his excellent book on the "Chemical Aspects of Immunity," remarks that "these figures give a striking illustration of the delicacy of the immunological methods and their value in studying certain problems in protein chemistry. In no other way would such minute amounts of protein be detected in a solution."

This statement, which expresses the conditions actually prevailing, led me to make some preliminary experiments which are reported in this paper and which show that it is possible, by a purely physical method, to detect the presence of still smaller amounts of protein, namely, 0.00000002 gr. or 1/50000000, in two hours or even less.

This method is based on the use of the tensiometer previously described[1] and on a phenomenon mentioned in 1922[2] and later in this journal,[3] under the title "Antagonistic action of colloids."

When a trace of powdered sodium oleate is added to pure water, or to a salt solution contained in a watch-glass, the surface tension decreases instantaneously and becomes very small. If the amount of sodium oleate added is smaller than 1/1000, the drop will continue for over one hour until a certain minimum value is attained. This value will then remain practically constant.[4] But when the same amount of sodium oleate is added to the same watch-glass containing some other colloid in solution, instead of pure water or saline, the surface tension, after reaching almost instantaneously its bottom value, starts up immediately and in a few minutes, according to the concentrations, tends towards its original value or even reaches it.

The presence of 1/1000000 of a gram of protein has no effect on the value of the surface tension of water, or very little, as the monomolecular layers are formed for egg albumin, for instance, at dilutions comprised between 1/80000 and 1/210000. Consequently it was thought that an indirect method, based on the above described "antagonistic phenomenon" was more likely to bring results. For this purpose a solution of 1/300000 of pure sodium oleate was prepared, and 1 cc was poured in a watch-glass; on the other hand, 1 cc of solution of 1/25000000 of crystalline egg albumin was added to it. The final dilution of the solutes was consequently 1/600000 for sodium oleate and 1/50000000 for egg albumin. Then, the drop of the surface tension of this mixed solution was compared with that of a solution of pure sodium oleate at 1/600000. The results of three series of experiments were as follows:

SURFACE TENSION IN DYNES

	Initial values			Static values (2 hours)		
No. of experiment	1	2	3	1	2	3
1. Water	75.4	75.3	75.3	75.3	75.3	75.3
2. Egg albumin crystal. 1/50 000 000	75.1	75.1	75.2	75.3	75.3	75.3
3. Sod. oleate. 1/600 000	75.0	74.9	74.7	70.8	72.0	71.0
4. Sod ol. + egg albumin same concentrations as above	75.0	75.0	75.0	75.1	75.2	75.2
Difference between the static values of Sol. No. 3 and 4, due to the presence of egg albumin				4.3	3.2	4.1

Consequently, the presence of 1/50000000 of egg albumin in 2 cc of liquid will prevent the lowering of the surface tension of water by sodium oleate in the proper dilution. This phenomenon is observed only when the sodium oleate is highly diluted. It would certainly be at a maximum for a concentration of 1/750000 in the watch-glasses which we use, filled with 2 cc of solution, as under such conditions, I have shown that an organized monolayer is formed.[5] However, it is difficult to prepare sodium oleate in such state of purity that it will entirely dissolve in true solution, and it happened that the sample used in the above reported experiments was not absolutely perfect. Nevertheless, the phenomenon is quite striking and clear at 1/600000. It is very probable that the sensitivity of this reaction can still be increased, and we have already secured evidence that it might show the presence of 0.00000001 gram of protein. Experiments are now being carried on to study the phenomenon more thoroughly.

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